natural selection as sexual selection, correlated variability, the law of homology, or any other of the Darwinian factors. The expression used by Mr. Romanes for his new factor—the "Segregation of the Fit"—seems to imply fitness for something, presumably for the conditions of life, and if the survival of the "fit" race is determined by natural selection, then I venture to think that natural selection must still be regarded as *the* theory of the origin of species and as something more than a theory of the origin of adaptations.

To the foregoing Mr. Romanes will probably say that physiological selection is a *necessary* adjunct of natural selection, and that no new species can arise without the co-operation of his factor. If this be the case, then, bearing in mind the views which I have expressed with reference to the subordination of physiological to natural selection, it seems to me that the most likely course to pursue is to appeal to the latter for an explanation of the "primary" specific character, viz. sterility. It is true that Darwin and many of his followers have attempted in vain to account for this primary specific distinction by natural selection, but I still venture to think that the solution lies in this direction. Indeed, the elements of the solution appear to me to be furnished by the original theory of Darwin and Wallace, as I will now briefly attempt to show, hoping to elaborate the idea on some future occasion, or, still better, leaving it for development or extermination in the hands of professed biologists.

The struggle for life being the most severe between the most closely allied forms, diversity is in itself an advantage, because the individuals which depart from the parent type may (but not necessarily *must*) "seize on many and widely diversified places in the economy of nature, and so be enabled to increase in numbers." Hence Darwin's principle of "divergence of character," so well restated by Mr. Romanes in his paper. Now, if diversity is an advantage, natural selection can deal with it like any other advantageous character, and *would seize upon any means afforded to secure its perpetuation*, provided always that the divergence was in the direction of some unoccupied "place in the economy of nature." This last condition amounts to nothing more than that the divergence is to be of an advantageous character. Among the most effectual means of securing permanent diversity must be sterility with the parent form: hence, given a variability in the reproductive capacities of different pairs of individuals (which I have already conceded), the question is whether natural selection could not develop out of this more or less imperfect fertility a more or less complete sterility. If it is to the advantage of some particular variety not to resemble its parent form, out of the immense number of divergences which are always taking place (by ordinary variation) those varieties which showed the greatest infertility with the parent form would in the long run survive, for the very reason that their progeny, tending to inherit the characters of their parents, would possess the advantageous characters of the latter, which led to their survival in the first instance, and, among these characters, that diminished fertility with the parent form which lessens the chance of their extermination by intercrossing.

As the foregoing argument is necessarily expressed in general terms, it will be of use to specialise our ideas by an appeal to a hypothetical case. Suppose, for instance, that a dominant species gives rise to the twelve varieties A, B, C, ... L, out of which four, B, D, K, L, possess some slight advantage over the parent form, adapting them to some new conditions in their environment. The four varieties thus stand a chance of surviving, while the eight others would be the "unfit." Now these four varieties in their incipient stage (and in the absence of isolation) would be subject to extermination by intercrossing in the next generation with the parent form. But the chances against these four varieties being *equally fertile* with the parent form must be exceedingly great: let us suppose, therefore, that B and K are less fertile with the parent form than D and L. Under these circumstances the latter would be wiped out by intercrossing, while the former would tend to retain their peculiarities and thus survive. The peculiarities both of B, K, and D, L, were originally advantageous, but those of B, K, are alone allowed to survive. The parent species has, as it were, attempted to give rise to four derived species, and has only succeeded in producing two. It is a case of selecting out of a number of advantageous modifications those particular varieties which are the least fertile with the parent form. From the slight sterility thus produced in the initial stages, natural selection, by acting in the same direction, might evolve the more or less perfect sterility which we now behold, because every departure on the part of the derived form in the direction of fertility with the parent form would be a retrograde step tending to obliterate those advantageous characters which led to the first survival of the new form, and the descendants of such partially fertile departures would constantly be weeded out owing to the dilution of their peculiarities with the less advantageous characters of the parent form. To put the case in another way, it may be said that natural selection is constantly endeavouring to develop the most advantageous modifications of every species, but succeeds the better the less the degree of fertility of the advantageous modification with the parent form, and succeeds only perfectly by producing perfect sterility with the parent form. Fertile advantageous modifications, on the other hand, would be swamped by absorption into the parent form.

I thus venture to think that the theory of natural selection as sketched out by Darwin and Wallace is still a theory of the origin of species. The production of the sterility of species by this agency is, according to the present views, to be referred to the same causes as all the other modifications produced thereby, viz. the natural selection of a "spontaneously" occurring variation in the function of one particular organ. In the case of domesticated races no such selection with reference to the functions of the reproductive system has been effected, but the varieties have only been kept from interbreeding by what amounts to isolation. It is not surprising, therefore, that such artificial forms, which have been selected only with reference to *external characters*, should be fertile *inter se*, while natural species, in which fertility *inter se* has been rigorously suppressed by natural selection through long series of generations, should exhibit a greater or less degree of sterility.[1] In other words, "physiological" appears as one particular phase of natural selection, and as far as we can see there is no reason why there should not be other modes of variation leading to the same result by acting indirectly upon the reproductive system. But all such modes of variation would still be subject to development or suppression by natural selection. R. MELDOLA

Greenock, August 21

THE Duke of Argyll's letter about organic evolution, published in your last week's issue (p. 335), calls for a few remarks, as it is very misleading, and bespeaks some misconceptions on the part of the writer. He has evidently read his own views into the two articles on organic evolution contributed by Spencer to the April and May numbers of the *Nineteenth Century*. In those articles Spencer makes no "admission"; what he says there with respect to natural selection has been held by him for the last twenty-six years. He does not deny that the natural operations denoted by natural selection do constitute an operating cause in the evolution of species. Only, he goes deeper: he, with his characteristic truly philosophic insight, sees in natural selection a *proximate* cause; sees behind it the primordial operations of forces of nature which rendered natural selection possible, and supplied it with a *point-d'appui*. Then he assigns use and disuse as another cause in the origination of species. Now all this is not a "declaration against" what your correspondent pleases to call "Mechanical Philosophy," but is a part and parcel of it. It is rather a "declaration against" all sorts of teleological philosophy. Let him remember also that Spencer's philosophy is the acknowledged philosophy of evolution; and he may rest assured that, even if the theory of natural selection *as a cause* in the genesis of species be proved untrue, that philosophy will still stand opposed to any philosophy that will attempt to bring back "Mind" as one of the *causes* of organic evolution.

Your correspondent is a little too hasty in his rejoicings over Mr. Romanes' paper on "Physiological Selection." He will see from the second part of the paper that even Mr. Romanes is unable to deny that in some cases at least natural selection is quite competent to originate species.

Then your correspondent thinks the theory of natural selection "not only essentially faulty and incomplete, but fundamentally erroneous," "in so far as it assumes variations to arise by accident." Now by "accident" or "chance" in this connection, evolutionists (including Darwin) have simply meant the action of some hidden physical causes whose exact mode of operation is not known. We all know, however, that variations are *facts* of nature, and it is not difficult to see that they are the necessary consequences of the varying number, amount, proportion, and manner of action, of the natural forces acting on different portions of living matter. Now, in making variations the starting-point, the theory of natural selection may justly be considered to be "incomplete," even as our knowledge of electricity is incomplete because we do not know the real nature of the thing, as astronomy is incomplete because we do not know for certain how, for instance, the solar system was formed; but in so doing the theory cannot be "essentially faulty" or "fundamentally erroneous." It is illogical, not to say childish, to think a theory to be erroneous because it cannot render a definite explanation of some unquestionable facts of nature on which it is based and with which it starts. To prove the theory of natural selection to be erroneous, it must be shown that it is never competent to originate species. If it ever falls, it will fall quite *ir*respectively of its avowed inability to give definite explanations as to the exact mode of occurrence of variations. S. B. MITRA

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[1] From the above it follows that local races or species produced by isolation should be more or less fertile with the parent form. This is a point which might well be tested experimentally.

19, Keppel Street, Russell Square, London, August 17

Red Sunsets and Volcanic Eruptions

PROF. S. NEWCOMB's article on the above subject in NATURE of August 12 (p. 340) induces me to send you a brief account of the atmospheric phenomena that I observed in Palermo during and after the recent eruption of Mount Etna.

This volcano is 133 kilometres distant from Palermo, but the transparency of the air here is so great that it is almost always visible from this Observatory.

At dawn on May 21 the smoke from the eruption appeared as a great mass of black vapour, rising from the southern side of the volcano. At 11 a.m. it had formed into rosy balls of vapour, or cumuli. With the theodolite I measured the angular height—2° 21′, which gives 8 kilometres of linear altitude. On May 24 the smoke had the characteristic form of a pine-tree, and a greater height, but at 4 p.m. the upper edge of it was not well defined, and I obtained (approximately) the altitude as 14 kilometres.

Since May 22 these vapours from Etna have spread over the eastern, and more recently over the entire, horizon of Palermo. In the early morning of June 3 the fog was so dense that the sun was invisible, and the towers of the Matrice, 200 metres distant, were only indistinctly visible, which in Palermo is quite extraordinary. From May 29 to June 3 Italy has been invaded from south to north with mist, which was probably also derived from Etna.

Cinders from the volcano have fallen over Eastern and Southern Sicily, and over Calabria, as well as in Palermo, where, in the dust gathered on the terraces of the Observatory on May 27, I detected with the microscope some minute crystalline laminæ of labradorite, which mineral is characteristic of the ejections of Etna.

The sun rising from the sea behind these mists has been purple-red and then reddish-yellow; at a height of about 30° it was neutral gray, but never green or blue. In Nicolosi, too, on the side of the volcano, these colours of the sun have not been observed. The light of the red sun was so faint that it was possible to look at it without inconvenience.

No corona (like "Bishop's ring") was observed around the sun or moon. Spectroscopic observations of the red sun gave only the ordinary atmospheric absorptions, perhaps somewhat intensified.

In the latter part of May and during the month of June the red after-glows appeared almost daily, and were stronger than before or since, but they were not so brilliant and prolonged as in 1831 and 1883–84, and their colour was not properly rosy, but an impure reddish-yellow.

I believe that the red sun was caused by the finest cinders from the volcano, suspended in the air, as the like phenomenon is produced by the dust of the Höherauch in Northern Europe, of the Sirocco in Sicily, and of the Kamsin in Africa. The blue sun (observed after the eruptions of Ferdinandea and of Krakatão) has not appeared, and the after-glows were not strongly brilliant, because the vapours ejected from Etna were not so enormously abundant as those ejected from Ferdinandea and Krakatão, which are marine volcanoes more directly communicating with the water of the sea.

The observation by M. Janssen, mentioned in NATURE of July 29 (p. 299), of a blood-red coloration of light traversing dust, gives a strong confirmation to the preceding explanation of the red sun. A. RICCO

Palermo Observatory, August 17

P.S.—Since July Bishop's ring has not been visible in Palermo.

The Bright Clouds and the Aurora

ON the morning of the 11th inst. I had an opportunity of watching the curious cirrus-like clouds as daylight came on. The display was striking, though not such a bright one as on several former occasions. It first appeared about 2.30 a.m., when there were very faint indications of the clouds; it was some minutes before I noticed that they were the same brilliant kind as has appeared so often this summer. Their apparent upper border being irregular, it was uncertain whether they in any part reached the limit to which the sun could shine upon them, or whether the apparent border was altogether the actual edge of the cloud-sheet; however, it rose higher as the sun approached the horizon, but this might be owing to the circumstance that the motion of the clouds was, as usual, from an easterly direction. At 3.33½ a.m. they were visible as far as α Andromedæ, though they were very faint west of γ. By 3.45½ a.m. they reached down to within 5° of α Aquilæ, and were rather plain there, and by this time the sheet covered most of the sky, though none of it remained visible very low down in the east. It was no longer bright in any part. At 3.55½ they reached down to within 4° of α Aquilæ, and were plainest about there, but growing fainter. I was still uncertain whether the sheet extended beyond the western apparent border, that being simply the limit of sunshine, or whether the sheet ended there; but probably the former was now the case. At 4 a.m. they were scarcely noticeable, and by 4.11 they had disappeared altogether. By this time a faint pink glow had appeared in the east.

The question is, Was the disappearance of the clouds due to their having evaporated, and ceased to exist, or to their light being overpowered by the brightness of the sky? It appeared to me that the latter was the case. It will be well if further observers can confirm this supposition or otherwise; if correct, they cannot be considered clouds at all in the ordinary sense, the sky being beautifully clear and blue after they had ceased to be visible. I could not say at any time that the clouds were not perfectly transparent to the stars. The circumstance that they have never been described as having been seen by day seems confirmatory of the above supposition.

With respect to Prof. Smyth's remark about the spectrum (p. 311) I do not gather whether he considers that the auroral line noticed by him belonged at all to the clouds or entirely to the aurora; but I think that there can be no doubt that the latter was really the case. He does not seem to have detected any aurora at the spot where the clouds were seen, but doubtless it was there, although overpowered by their brightness. As it is so evident that these clouds were illuminated by the sun (this being confirmed by their varied colours depending on their altitude, as described by Prof. Smyth), we cannot expect their spectrum to be otherwise than solar and atmospheric. I looked at them with a miniature spectroscope on the evening of July 12, as well as on the morning of the 11th inst., but on neither occasion was the spectrum bright enough for me to perceive much. I could not see any lines, bright or dark, but the spectrum faded very abruptly in passing from green to orange, which no doubt was owing to the atmospheric bands near D, especially the "low sun band."

On July 27 I saw the aurora mentioned by Prof. Smyth at Gilsland, in Cumberland, and it was a particularly magnificent one there, especially about 10.25 p.m., at which time a part of it was lilac—a very unusual colour. The bright clouds were also visible that night, but chiefly before the aurora appeared and after it vanished; there appears no reason to suppose there is any connection between the two phenomena.

As regards the dark space beneath the auroral arch, has the theory mentioned by Prof. Smyth ever been proved, that there is any true darkness there, and that it is not merely the effect of contrast with the aurora? My impression is that it must be at least mainly the effect of contrast, though perhaps not entirely, and the idea is confirmed by a similar darkness sometimes appearing by contrast with the brilliant clouds, when no